

The work concludes with a chapter of "Results," which shows that illusion in general consists in a "bad grouping of psychical elements," and as such shade off into fallacies of reasoning; in both there is a want of correspondence between internal and external relations. In the future and for the race natural selection and "direct equilibration" can only be expected to remedy the sources of any such mal-adjustment in so far as it may be of actual injury to life. Thus we can have no absolute criterion of illusion. "Science cannot prove, but must assume the coincidence between permanent common intuitions and objective reality. To raise the question whether this coincidence is perfect or imperfect, whether all common intuitions known to be persistent are true, or whether there are any that are illusory, is to pass beyond the scientific point of view to another, namely, the philosophic." This consideration leads to an exceedingly able statement of the relations between scientific and philosophic thought, but the discussion necessarily runs into an abstruseness that it is not desirable here to enter. In general, however, it may be said that in this, as in some of his other works, Mr. Sully shows that while he has perceived more distinctly than most of our leading psychologists the sharpness of the boundary between science and philosophy, he displays an admirable clearness of thought in never allowing the methods of the one sphere to encroach upon those of the other, while in whichever sphere he chooses to work he enjoys the privilege, almost unique among psychologists, of finding himself equally at home.

GEORGE J. ROMANES

OUR BOOK SHELF

Studies in Biology for New Zealand Students. No. 1.—The Shepherd's Purse (*Capsella Bursa-Pastoris*). By F. W. Hutton, Professor of Biology, Canterbury College, University of New Zealand. (New Zealand: By Authority. 1881.)

THIS is a detailed study of the coarse and minute anatomy of a very familiar and widely-diffused weed. A native originally of the palæarctic region, it has now found its way to all temperate climates. It has certainly received at Prof. Hutton's hands, in the Antipodes, a more systematic investigation than ever fell to its lot in Europe. The treatment is much the same as that given to the bean (*Faba vulgaris*) in Huxley and Martin's "Elementary Biology," though with a more botanical bias. The weakest part is the treatment of the root, where nothing is said about the mode of origin of branches. An earlier stage should have been taken, showing the arrangement of the fibro-vascular tissues before they had coalesced into a central cylinder. This however is simply by way of criticism. The method of treatment is excellent, and the Canterbury students are fortunate in being in the hands of a teacher who has such a thorough appreciation of the biological method as applied to botany.

A Text-Book of Indian Botany, Morphological, Physiological, and Systematic. By W. H. Gregg, Lecturer on Botany at the Hugli College. (Calcutta: Thacker, Spink, and Co., 1881.)

THIS is the first eighty pages of a book which, when completed in 500-600, will apparently be practically Henfrey's Elementary Course, adapted to the local requirements of Indian students. There does not seem anything particularly novel or noteworthy in the treatment of the subject, as far as can be judged from the portion printed. As is usual in books of this type, some space is devoted to the

Linnean classification. But, as the author points out, there is the excuse that Roxburgh's *Flora Indica*, which is still unsurpassed, is arranged in accordance with it.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Re W. I. Bishop

I CANNOT but feel greatly surprised that Mr. Romanes, when reporting the result of the investigations made by his colleagues and himself upon the power of "thought-reading" claimed by Mr. W. I. Bishop, should have stated that the letter of introduction which I gave to Mr. Bishop was "doubtless intended to recommend him to the attention of the credulous," since this letter most distinctly expressed my desire to obtain for him "an assemblage of gentlemen specially qualified to appreciate the importance" of what I described in it as (in my judgment) "experiments of great value to the Physiologist and Psychologist." Nor can I see how my having thus recommended "him to the attention of the scientific" is a thing "to be regretted"; since the careful testing of the one set of experiments which Mr. Bishop has shown to Mr. Romanes and his colleagues has resulted in a precise confirmation of my statement that the power of "thought-reading" which he claims is "derived from his careful study of the indications unconsciously given by the subjects of his experiments, and from his peculiar aptness in the interpretation of those indications."

What I think "is to be regretted" is that Mr. Bishop did not offer for like careful testing another remarkable set of experiments which he had repeatedly performed in the presence of distinguished medical and scientific men in the United States (from whom he brought introductions to me), and also before a like assemblage in Edinburgh; showing his power of naming words and numbers previously written and sealed up in private, by his acute recognition of indications unconsciously given by their writers when the alphabet or digits were "called."

As I have never credited Mr. Bishop with any other power of "thought-reading" than this, I have been surprised to learn that I am accused of "fathering a new humbug."

Another "experiment" which he performed in my own house some time ago struck me as well worthy of careful testing:—

The "subject" of the experiment being asked to draw a card from the pack, to identify it, and then to return it to Mr. Bishop, the latter, after shuffling the pack, dealt out sixteen cards *with their faces downwards*, arranging them in four rows (which I indicate by letters and numbers), as thus:—

	A	B	C	D
E	1	2	3	4
F	5	6	7	8
G	9	10	11	12
H	13	14	15	16

The "subject," having been caused to stand at the table with the cards directly before him, Mr. Bishop, standing at his right side, and taking his right hand into his own left, said to him, "Drop your left hand down on either row (whether *horizontal*, as A, B, C, D, or *vertical*, as E, F, G, H), that you wish taken away." Row B having been selected and taken away, there remained the three rows, A, C, D. "Now," said Mr. Bishop, "drop down on another row." Row D being selected, there remained rows A and C. "Now drop down on a third row." Row A being selected, there remained only row C. "Now drop down upon either the two upper or the two lower cards of the remaining row." The two upper (3 and 7) being selected and taken away, there remained only the two lower (11 and 15). "Now drop down

upon either of the two remaining cards." The lower (15) being selected, and the remaining card (11) being turned up, this proved to be the card originally drawn.

Having seen this experiment twice made successfully with members of my family, I offered myself as the next "subject" of it, with the determination to watch carefully for any manual guidance by which Mr. Bishop might be influencing my choice. The experiment succeeded with me as it had done with my predecessors, and yet I could not, any more than themselves, tell how I was led to make the five successive selections of the cards to be taken away, so as to leave behind the card I had originally drawn.

It may, of course, be assumed that Mr. Bishop knew where he had placed this card, although his "subject" did not; and he informed me that experience has taught him the positions to which the choice of his "subject" can be most easily and certainly guided. The influence of the eyes being excluded by the relative positions of Mr. Bishop and his "subject," the guidance *must* be conveyed through the hand which Mr. Bishop holds in his own; and yet I altogether failed to detect the mode in which it was given.

Of course it may be said that this is only a variation of the conjuror's trick of "forcing" the card which he has determined that the drawer shall choose. (I remember seeing it stated that Louis Napoleon, when Emperor, had defied Houdin then to "force" a card upon him; and that Houdin made him draw the card which in the French pack is designated Cæsar.) But though the same principle of "suggestion" is involved, the conditions under which it acts are altogether different. The conjuror stands *opposite* the drawer, looks at his face as well as at his hand, and continually shifts the position of the card he holds, so as to prevent a *wrong* card from being drawn, while presenting the *right* one in the manner which he knows by experience to be the most suggestive. But Mr. Bishop does no such thing. The cards remain in their places with their faces downwards; and of the guidance given him by Mr. Bishop standing at his side, the "subject"—even when on the watch for it—remains quite unaware.

If I have made myself understood by your readers, I think I shall have satisfied them that this "experiment" (which may by no means invariably succeed) is of great psychological interest, as showing the large measure in which we may be guided in our choice among things "indifferent," by *influences of which we are ourselves unconscious*.

WILLIAM B. CARPENTER

American Meteorological Observations

IN your valued journal (vol. xxiv. p. 16) I find an expression of your regret that it should have been decided that the printing of the *Bulletin* of Simultaneous Meteorological Observations should hereafter take place one year after date, instead of six months. It may be interesting to those of your readers who made use of the *Bulletin* in studying the general atmospheric phenomena of the northern hemisphere, to know that for several years past the data for several distant land stations in Greenland, Iceland, Siberia, Alaska, &c., have been omitted, merely because the mail facilities did not enable us to receive the reports in time for publication in the *Bulletin*. Thus a large portion of the region covered by our maps has been left unrepresented, for which the necessary data come regularly to hand a few weeks or months later. The case is still worse in reference to the marine reports for vessels off on long voyages; for instance, we regularly pay for and receive a large collection of material from the London Meteorological Office that never appears in our published *Bulletin* or charts. The proposed postponement of publication is in fact merely the outcome of several suggestions and recommendations from co-operating nations, in the propriety of which recommendations myself and assistants fully concur.

W. B. HAZEN,

Chief Signal Officer, U.S.A.

Office of the Chief Signal Officer, Washington, D.C., June 15

A Meteor

LAST night, June 24, at 11h. 29m. G.M.T., I observed a meteor, as bright as Jupiter, cross the tail of the great comet 4° or 5° above the head and disappear some 20° to the left, on the vertical of Beta Ursæ Minoris and at an altitude equal to that of the comet's head. It left a bright streak for some seconds. I did not see the beginning, and perhaps not the end, as it may

have continued behind a cloud bank. The duration for the above path was three seconds, determined afterwards by experiment. Place of observation, lat. 51° 32', long. 0° 11' W.

G. L. TUPMAN

27, Hamilton Terrace, St. John's Wood, June 25

LOOKING at the comet last night from my garden at 11.25 p.m. I saw a large meteor pass nearly horizontally from a little east of north to within a short distance of the comet, rather above the head. It was as large as Venus when brilliant, but with a red or orange tinge. The motion was rather slow.

A.

Adsett Court, Westbury-on-Severn, June 25

Earthquake in Van

IT may perhaps be considered worthy of a note in your columns that an earthquake was experienced in this neighbourhood on Monday, May 30, at a few minutes before 6 a.m.

Here in Van the shock was slight, consisting only of a tremulous motion lasting a few seconds; but I have to-day received information that at Bitlis the shock was so severe as to cause people to rush out of their houses in fright, and that a village named Téout, situated near the western shore of the Lake of Van, was destroyed by it, with the loss of a considerable number of lives. I have as yet heard no details, but if any further circumstances of interest should come to light I will communicate with you again.

It is well known that the environs of the Lake of Van show many signs of ancient volcanic action; at least three volcanoes with distinct craters forming prominent features on or near its shores. Of these the Nimroud Dag, on the western shore of the lake, is said by tradition to have been active not more than 400 years ago. It contains an immense crater five or six miles across, in which are situated hot springs. The village Téout of which has been destroyed lies at the foot of the eastern slopes of this mountain.

EMILIUS CLAYTON

Van, Turkey in Asia, July 6

Freshwater Actinææ

I YESTERDAY noticed in a small *freshwater* aquarium four specimens of a small freshwater Actinia of a very pale olivaceous colour. They have each six tentacles more than 1 inch in length when fully extended, but then so extremely fine at the ends as to be almost invisible. The body or stalk is about 0.1 inch long by 0.05 inch in diameter when at rest, and about 0.5 inch long and 0.015 inch in diameter when expanded.

I was not aware before of the existence of freshwater Actinia, but as the specimens to which I now refer are in all respects similar to sea-anemones, there can be no doubt on the subject.

I have succeeded in transferring two specimens, which have duly rooted and expanded themselves in a bottle and a tumbler, and I shall be happy, if they are of sufficient interest, to send one to your office or elsewhere for inspection.

W. SEDGWICK
Royal Naval School, New Cross, June 24

The Observation of Hailstorms

IN the most casual survey of the literature relating to the phenomenon of hail one cannot fail to be struck with the remarkable contradictions which everywhere make themselves apparent. Some writers say that hail falls oftener in the tropics; others assert that it is altogether unknown there! Howard states that the maximum hailfall in this country occurs in the summer season, while Dalton and others say that it is in winter. I think these singular discrepancies are attributable, in many cases at least, to imperfect observations. Most of the meteorologists who have given special attention to the phenomenon of hail have had their pet theories, and naturally their observations have been guided to a considerable extent by the requirements of the particular theories which they advocated. Thus we find Kämtz ascribing the formation of hail to the conflict of opposing currents of wind, Volta to the electrical condition of two separate layers of cloud, Leslie to the presence of strata of air at different temperatures, von Buch to ascending currents of air, and so on, all which theories are based, not upon invariable phenomena, but upon isolated features which happen to have come repeatedly under the notice of those particular observers in the particular hailstorms which they witnessed. I think it would enhance the